class is *three-fourths of the half-wave length*, and then in accordance with varying diameters above stated all larger diameters demand length to be increased in ratio, and more allowance to be made for the diverging than for the uniform bore; greater or less *amount* of wind, and greater or less *degree* of pressure also enter into calculation, and practically are convertible in effect, the one doing duty for the other.

In the distinctive mode of action of this class we may find reasons for the varying relations of the pipes to each other, and for the contrasts shown in comparison with the other class acting under its own peculiar mode. In these propulsive pipes in both the wide and narrow scales, the wind-current, after entering the foot of the pipe or boot, passes into the body of the pipe by a very contracted inlet formed by a hollow plug usually of metal called "the beak," or more commonly "the reed," to the confusion of inquirers; properly named, as we see it in old authors, it is "the shallot," from its resemblance in shape to the once favourite esculent the eschallot; ordinarily we speak of "the tongue" the elastic strip of metal covering it, as "the reed," for in the clarionet this part is really a reed. The main impulse of the current passes into the cone of the pipe through the mass of air in a central direction, and thus in a wide pipe, as compared with its course in a narrow pipe, the current has exchanged the friction upon the sides for the lesser friction of air upon air, still restricted, but less so in degree as the cone expands, as of a swift river escaping the confinement of banks, flooding the quiet expanding delta, agitating its waters with gradually-decreasing strength, and then becoming diffused in surrounding ocean.

Utmost exactness in length is quite as important for pitch and tone in these as in flue-pipes. Although the reed tongue has a determinate pitch of itself, yet a proper length of tube to reciprocate its action is indispensable, any inaccuracy only "upsets the tone," as the technical phrase says, and gives rise to curious freaks of behaviour. The slim tapering oboe is so sensitive that if we make it a quarter of an inch too long, or if we merely pat the top of its bell whilst sounding, the tone will immediately leap to its third above—not to a harmonic—a problem as puzzling as that of some echoes falling successively by thirds.

The action of the air-reed as causing suction by the velocity of passage of wind over the mouth was illustrated by me in a previous paper by reference to the abstracting power of a current of air, as shown in the spray-diffuser where globules of liquid are lifted and withdrawn by its agency. The action in these beating reeds is also susceptible of as simple an illustration. Take six or eight feet of india-rubber tubing of $\frac{1}{8}$-inch bore, for this length defines action more clearly—coil the length round your hand, and placing one end in the mouth blow through the tube sharply, at the same time allow the tip of your tongue freedom of play near the orifice, and you will find it drawn suddenly to the tube by the suction of the current passing down it, and released only on the exit of the current into the atmosphere. Lightly pressing the coil in your hand you may likewise feel the throb of the passing air-pulse. The trombone-player is conscious of his lip being forcibly drawn into the cup by a like cause. A stream of air suddenly cut off cannot pass down a tube without leaving a vacuum behind it. Organ and orchestral trumpets and oboes, and all of like propulsive action, are subject to this power, and only through it are able to generate tones. Suction is thus seen to be the final cause of vibration, the vacuum exists until the initial pulse of the vibration has made exit at the outer orifice, or in the second and succeeding courses until the pulses reach the colliding point or place of the prime node. Always thus in every musical pipe the current is essential to the suction, but with the striking difference that in the flue-pipe there is continuity of stream, and the continuity of flow is made available by conversion in reciprocating motion, but in the propulsive class the action is effective through discontinuity, by abrupt cessations and sequences of stream.

Here also in the beating-reed pipes we come upon distinct evidence of the interval of rest lengthening the period of vibration. The pitch of beating-reeds is regulated to consort with the pipe by means of a tuning-wire altering the vibrating length of the tongue; thus regulated, the pitch may, however, within limited degree be altered by changing the force of wind, or by cutting off rim of pipe, or by adding thereto. Let it be observed that whether the tongue is pressed to the beak slowly or quickly, it will spring back in recoil in just the same time. By additional weight of wind, pitch may be raised, and in this case the tongue flies to more rapidly, but possibly any gain of speed in the advance may be counteracted by the recoil being impeded in the more compressed medium in which the tongue moves; the only remaining effect otherwise is that of an increased swiftness of the current of air more vigorously propelled in its course, and this in itself would account for the acceleration of pitch. On the other hand, leaving the force of wind constant, we may by temporary addition to the rim of upper orifice sensibly flatten the pitch, for the current takes longer time to pass this extra boundary, hence the tongue is in consequence held longer upon the beak by the suction, its recoil delayed, or in other words recognising the physical result, its interval of rest is lengthened.

Many indications that come before me in my experiments lead directly to the inference that in all wind instruments this interval of rest is an important influence both on the pitch we regulate and in the quality we perceive; and in the estimate I shall have to give of the interior process of working in the flue organ-pipe, I shall draw upon this inference that vibration is an activity tempered by rests.

One point has been unnoticed. It would be easy to find a diapason-pipe of the same pitch showing precise agreement in length with the trumpet above specified, and similarly for other various kinds. The recognition of numerous like correspondences has led to the supposition that in relation to wave-length these two classes of pipes exhibit a parallelism. I hope to have made it clear that on the contrary, never parallel, the two classes proceed on two distinct systems of relation to wave-length, and are governed by a law, simply expressed as a law of divergent variation; they meet, it is true, but only at one point, where they cross in divergent lines, and they develop in opposite phases both in the ascending and descending extents of their range, the pitch of the one rising under an enlarging, and the other under a diminishing diameter.

Beyond the particular effects of friction already stated, the agency of the friction of air in the sound of wind instruments appears to me inadmissible. Reasons for this conclusion will occupy another paper in connection with details of my experiments bearing thereon under a simple device somewhat on the principle of the siren, and which may be named a "displacement siren."

HERMANN SMITH

Solar Halo

ON Saturday last at Penruddock station, between Penrith and Keswick, about one o'clock, I observed a solar halo which at first was not perfect, but showed a reddish tint in the arc below the sun. Afterwards the circle became complete and continued so with small intervals until about half-past four, when I went indoors. At five o'clock the halo had disappeared in the haze. The day was thick, so hazy indeed that I could hardly distinguish the outline of Saddleback from Penruddock. The colour disappeared when the circle was complete, but occasionally I thought I could distinguish a reddish tinge on the inner side of the arc. I had no means of accurately measuring the radius, but with two pieces of stick which I picked up I estimated the tangent at $\frac{4}{9}$, which would give nearly 24°. This is more than your correspondent Mr. Gledhill found in his observations, but my measurement is confessedly rough.

JAMES HEELIS

April 21

Safety Matches

MR. TOMLINSON's remarks on safety-matches in NATURE, vol. xiii., p. 469, reminded me that, not long ago I accidentally kindled one of those matches by rubbing it on the edge of a Wedgwood-ware mortar. This material appears even better adapted than those mentioned by Mr. Tomlinson for igniting such matches, and I found that a common earthenware dish (glazed inside) answered the same purpose admirably. I tried to ascertain the degree of certainty with which a safety-match could be kindled by friction against these two materials, and was surprised to find that they are little inferior in this respect to amorphous phosphorus itself. After a little practice in the manner of striking, it is easy to kindle nearly every match. Thus I have lighted forty matches out of fourty-four (most of them at the first or second stroke), using the glazed portion of the basin referred to. I should add that the surface becomes improved by use, which can hardly be said of the composition on the sides of the safety-match boxes.

Manchester, April 18 FRANCIS JONES

"The Ash Seed Screw"

THE delicate twist in the samara of the ash is clearly not that best calculated to retard descent. The more decided the twist,

the greater number of revolutions will the samara perform ere reaching the ground, and the longer consequently will be the path through which its friction is exercised.

In seeking a model for a screw-propeller, we must remember that the pitch should *vary* with the velocity of propulsion.

London, April 15 WM MCLAURIN

OUR ASTRONOMICAL COLUMN

THE ROTATION OF VENUS.—It was Jean Dominique Cassini (Cassini I. as he has frequently been designated) who during his residence in Italy, made the first serious attempt to ascertain the time of rotation of this planet and the position of the axis. His observations with one of Campani's long telescopes appear to have been commenced about the middle of the seventeenth century, as related in the *Journal des Savans*, 1667, Dec. 12, but it was not until the evening of Oct. 14, 1666, that he perceived any spot of sufficiently definite aspect to be of service for the purpose in view. It is described as "Une partie claire située proche de la section, et fort éloignée du centre de cette planète vers le septentrion." At the same time several dusky spots were noted. These observations were continued till June 1667, but Cassini expressed himself very cautiously with regard to the inferences to be drawn from them. They appeared to indicate a return of the bright spots to the same position upon the disc at intervals of about twenty-three hours, but from the short time that the spots could be followed Cassini was unable to decide whether the appearances were to be attributed to an axial rotation or to a libration. "De dire maintenant," he says, "supposé que ce soit toujours la même partie luisante, si ce mouvement se fait par une libration, c'est ce que je n'oserais encore assurer, parce que je n'ai pas pu voir la continuité de ce mouvement dans une grande partie de l'arc, comme dans les autres planètes, et par cette même raison cela sera toujours très-difficile à déterminer."

In 1726 Bianchini, domestic prelate of the Pope, observing at Rome, with glasses, also by Campani, of 70 to 100 Roman palms in focus, remarked on Feb. 9 several spots which he continued to observe with the view to determine the time of rotation. His observations were published in "Hesperi et Phosphori nova Phenomena," 1728, and he considered them to show a period of rotation of 24 days 8 hours, the North pole of Venus being directed to longitude 320°, with an inclination of 15° only to the plane of the ecliptic. Bianchini's observations appear to have been made under very unfavourable conditions, whereby he was prevented from following the spots in a continuous manner. They were discussed at length by Jacques Cassini, the son of Jean Dominique, in "Elemens d'Astronomie" (1740), who arrived at the conclusion that a rotation of 23 hours 20 minutes would represent equally well his father's observations and those of Bianchini, while if the rotation assigned by the latter was admitted, it would be necessary to reject entirely the observations of the elder Cassini, "comme n'etant qu'une apparence trompeuse."

Jacques Cassini mentions that after Bianchini had communicated to him the observations at Rome, he made attempts to discern the spots upon Venus at Paris. He examined the planet on a great number of occasions with a glass of 114 feet focus, one of the best produced by Hartsoëker, and also with one of Campani's, of 120 Roman palms' focus, which had been tried by Bianchini and considered excellent, but with all the precautions taken neither he nor Maraldi could perceive any distinct spot upon the planet's disc.

Schroeter, in 1789, examining Venus with a 7-feet reflector, discerned a bright spot in the dark hemisphere, and by following the appearance of this object, inferred that the planet rotated in 23h. 21m. 19s., thus supporting the result obtained by Jacques Cassini from his father's observations. Schroeter's observations appear in D. J. H. Schroeter, "Cythereographische Fragmente, oder Beobachtungen über sehr Betrachtlichen geberge und rotation der Venus," Erfurt, 1792; and in "Aphroditographische Fragmente, &c.," Helmstadt, 1796. In an appendix to the latter work, noticed by Zach in "Monatliche Correspondenz," xxv. p. 366, it is stated that observations of "atmospheric spots," and of the horns, with eight determinations of "a definite point upon the surface," give for the final value of the rotation-period of Venus, 23h. 21m. 7·98s.

De Vico's observations and investigations bearing upon the time of rotation and the position of the axis are published in "Memoria della Specola . . . in Collegio Romano," 1840–41, p. 32, and in the succeeding part of the same for 1842, p. 29. The period of rotation assigned from these observations, which were made with the Cauchoix refractor of the Roman College, is 23h. 21m. 21·93s. (sidereal time). The longitude of the ascending node of the equator of Venus upon her orbit is fixed to 56° 30′, and the inclination thereto 53° 11′, while for the same elements referred to the ecliptic we have 57° 19′ and 49° 57′. There is some error of the press or of calculation here which it is not easy to rectify. In a note to Secchi's Life of De Vico, "Memorie dell'Osservatorio . . . in Collegio Romano," Anno 1850, p. 140, the inclination of the equator of Venus to the ecliptic is given, 53° 11′ 26″, and the longitude of the ascending node of the equator upon the ecliptic for 1841, 57° 19′ 18″.

Notwithstanding the near agreement of De Vico's period of rotation with that assigned by Schroeter, it must be admitted that further investigation is very essential before we can consider the period established. There are so many negative observations upon record and these made under circumstances at least as favourable as those upon which the rotation of the planet has been supposed to be fixed, that there is ample justification for doubt in the matter.

We hear from more than one correspondent that dusky spots have been suspected upon the disc of Venus, within the last few weeks; if there be no illusion, the present may prove a favourable opportunity for attempting a new determination of the rotation-period, and this consideration has suggested the above outline of the actual state of our knowledge upon the subject.

MINOR PLANETS.—By a note from Herr Palisa it would appear that the small planet observed at Pola on November 22 and 23 was not, as supposed at the time, identical with the one he had detected on November 8. No. 155 is therefore lost or in similar predicament to the planet observed by Watson, 1873, July 29.

M. Leverrier's *Bulletin International*, of April 22, announces the discovery of another small planet at Paris, by M. Prosper Henry, during the previous night, in R.A. 14h. 9m. 58s., N.P.D., 102° 18′.

PROF. FLOWER'S HUNTERIAN LECTURES ON THE RELATION OF EXTINCT TO EXISTING MAMMALIA [1]

VIII.

THE existing families of the Carnivora, spoken of in the last lecture, do not appear to have been distinctly differentiated in the Eocene period, at all events not till towards its close, but the order was represented by other and very singular forms, the systematic position of which is not easy to determine. The earliest in point of time is *Arctocyon primævus*, from the lowest Eocene of La Fère, Aisne, France, an animal nearly as large as a wolf, with a long tail, and heavy, strong limb bones, and

[1] Abstract of a course of lectures delivered at the Royal College of Surgeons "On the Relation of Extinct to Existing Mammalia, with Special Reference to the Derivative Hypothesis," in conclusion of the course of 1873. (See Reports in NATURE for that year.) Continued from p. 488.